FIVE ENCOUNTERS WITH FELIX BLOCH

by Willis Lamb

ABSTRACT

The impact of Felix Bloch's work on the fields of parity non-conservation, the Mössbauer effect, nuclear induction, chemical shifts, and laser theory is described from a personal point of view.

I have benefited enormously from contacts with a number of the great theoretical physicists of the twentieth century. One such relationship far exceeds all the others in respect to duration and meaning to me. I am going to describe five encounters with Felix Bloch that involved purely scientific matters. Personal matters come into the account only to set the times and places of the episodes related. The first two and the last of the five encounters involved suggestions Felix made to me in connection with my research. If I had had the wit, energy, and luck to follow up properly the earliest two of these leads, I might have made some very good discoveries. The third encounter involved a request from him for help on his research. It turned out that he did not, after the fact, need this help. Still, the story has a certain interest for me and might provide a footnote for a history of modern physics.

The fourth encounter was very slight. I had worked on a certain problem in the early forties. Bloch was working in the same area in the early fifties. I had missed following up some interesting aspects of my work. Bloch and I talked about his problem and its relationship to my earlier research. It later turned out that his work was capable of enormous application, in chemistry rather than in physics, so that he did not follow it up as far as he might have done.

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The fifth encounter led me to the development of the theory of lasers and masers, which has been a very productive and useful field of research for over twenty years.

I received a B.Sc. at Berkeley in chemistry in 1934, and began the study of theoretical physics with Robert Oppenheimer. In those days, the Physics Departments at Berkeley and Stanford occasionally had joint picnics followed by a colloquium. One that I remember vividly took place in the spring of 1934 at the Lick Observatory on Mount Hamilton. The speaker was someone from Stanford named Felix Bloch. Solid state physics was not taught at Berkeley then, and I had no notion of the enormous contributions made to that field by the speaker. He had just come from Rome and told us about Fermi's new work on the theory of beta decay.

In the fall of 1934, Oppenheimer and Bloch began to conduct joint weekly theoretical physics seminars alternating between Berkeley and Stanford. During the next year I gradually got to know Bloch, who accepted me with an unusual degree of tolerance. In the summer of 1935, I went to the theoretical physics school at Ann Arbor. This was organized by George Uhlenbeck and Samuel Goudsmit and operated during the eleven years before the U.S. entry into World War II. Bloch was one of the lecturers that summer, and I learned from him the elements of his theory of electrical conductivity. I also acquired a little understanding of the theory of beta decay from other lecturers at the summer school, among whom were Enrico Fermi and David Dennison, as well as Uhlenbeck and Goudsmit.

PARITY NON-CONSERVATION

One day in the autumn of 1935, I was driving with Bloch somewhere in Berkeley, and he told me that the shape of a beta decay electron energy spectrum would depend on the change of angular momentum of the nuclear states. He suggested that I calculate this effect in more detail.

The angular momentum dependence contemplated by Bloch came from the variation across the nuclear radius of the plane wave electron and neutrino wave functions in the transition matrix element. This brought in additional momentum-dependent factors and thereby changed the shape of the theoretical electron energy spectrum.

At that time, the theory of beta decay was very primitive and poorly formulated. Competing with Fermi's originally postulated interaction for the emission of an electron and a neutrino was the proposal of Konopinski and Uhlenbeck that explicitly involved derivatives of lepton wavefunctions, and also changed the momentum dependence of the matrix element for the emission process. Gamov and Teller had also suggested an interaction that differed from Fermi's by the presence of nucleon and lepton spin operators.

By that time, I had learned from Pauli's 1933 *Handbuch der Physik* article[1] on Quantum Mechanics how he had classified the covariant quantities that could be made out of binary combinations of Dirac wave functions: scalar, polar vector, tensor, axial vector, and pseudovector. A particular combination of these interactions was used by Fermi, and, stimulated by Bloch, I began to investigate the effect on the beta spectrum of taking wild mixtures of the various Pauli building blocks. I remember that Robert Serber (then at Berkeley) showed me that some of the mixtures I was considering did not conserve parity! This might have been responsible for a missed opportunity, but 1936 was simply too early for the discovery[2] of non-parity conserving interactions. In fact, it took over twenty years before physics was ready for the theoretical analysis of Lee and Yang.

The experimental state of measurement of beta spectra in 1936 was very confused because of thick source effects. I never really accomplished anything very useful with beta decay theory, and the work was published[3,4] only in two abstracts of talks given before meetings of the Pacific Coast Section of the American Physical Society.

SUMMERS AT STANFORD

There wasn't much theoretical physics going on in Berkeley during the summers. Oppenheimer invariably spent about six weeks in the late spring at Cal Tech. I usually followed him there, but never to his summer retreat in New Mexico. Bloch stayed working at Stanford during the summers, and kindly provided me with a desk and library facilities. I was there during the summers of 1936–1939 and 1941. Each year there was a visiting lecturer, and in these years I came to know George Gamov, Edward Teller, Victor Weisskopf, J. H. Van Vleck, George Placzek, Samuel Allison, I. I. Rabi, George Kistiakowski, and many others who passed through Palo Alto on their summer travels. I also met most of the Stanford physicists: David Webster, Paul Kirkpatrick, Norris Bradbury, William Hansen, Russell Varian, Arnold Siegert, and Arnold Nordsieck (I had already known Nordsieck at Berkeley, and we later were colleagues at Columbia for many years). My meeting with Rabi led to an appointment as Instructor in Physics at Columbia in the fall of 1938 after I received a Ph.D.

THE MÖSSBAUER EFFECT

At Ann Arbor, I had heard Fermi lecture on the effect of chemical binding of a hydrogen atom on its scattering of slow neutrons. This interested me, and I began to work on related problems. It seemed that there might also be an effect of the binding of a hydrogen atom on the capture cross

section for slow neutrons. At first I thought the effect would be large, but finally had to settle for a very rough estimate of the cross section for a very unlikely process: the radiationless capture of neutrons by bound protons to form deuterons, with the excess energy and momentum going into vibrational motion of the deuteron instead of a gamma ray. The normal capture process was very little affected by the chemical binding. Even today, this radiationless capture has never been seen, but I am still hoping that someday it may be. This work[5] formed part of my doctoral thesis. The other part[6] dealt with electromagnetic properties of nuclear matter.

That summer, I came across a paper by Hans Bethe and Placzek[7] on the resonant capture of slow neutrons by nuclei in a gas. The resulting compound nucleus could decay in a number of ways that contributed to the natural width of the resonance curve. The Lorentzian resonance curve of the Breit-Wigner theory of capture of slow neutrons to form a compound nucleus was modified because of the Doppler effect. In the limit of small gas atom velocities, the resonance curve was Lorentzian, while for rapid motions it became a Doppler-broadened Gaussian. In the general case, Bethe and Placzek obtained the Voigt[8] profile, which was well known in the theory of Doppler broadening of naturally broadened spectral lines emitted or absorbed by atoms in a gas.

Since most of the experimental studies on resonant neutron capture involved atoms in a solid, I began to generalize the Bethe-Placzek theory to deal with nuclei bound in a crystal lattice. For the case of a simple harmonic oscillator (Einstein) model of the solid, it was clear that the neutron resonance curve would be a sum of Lorentzians with centers displaced by the separation of the vibrational energy levels of the oscillator. Then using what was learned from Bloch in a previous summer, I worked out the theory for a Debye model solid. Instead of a single oscillator frequency, there was a frequency continuum extending from zero up to a maximum value simply related to the Debye temperature of the solid. I derived a general, but complicated, expression for the neutron capture cross section as a function of neutron energy. The equations are reproduced here:

$$W(E) = \frac{2}{\Gamma} \text{ Real } \int_o^\infty d\mu \, \exp\left[i\mu(E-E_o) + \frac{i\Gamma}{2} + g(\mu)\right] \quad .$$

E is the neutron energy, E_o its resonance value without allowance for recoil, and Γ is the resonance width. The function $g(\mu)$ is given by a sum

$$g(\mu) = \sum_s q_s^2 \left\{(\bar{\alpha}_s + 1)e^{-i\mu\hbar\omega_s} + \bar{\alpha}_s e^{i\mu\hbar\omega_s} - 1 - 2\bar{\alpha}_s\right\}$$

over the normal modes s of the crystal lattice. The quantity $\bar{\alpha}_s$ is the thermal average number of phonons in mode s

$$\bar{\alpha}_s = \frac{1}{(e^{\hbar\omega_s/kT} - 1)}$$

where ω_s is the circular frequency and T the absolute temperature.

The dimensionless quantity

$$q_s^2 = \frac{(\vec{\mathbf{p}} \cdot \hat{\mathbf{e}})^2}{2NM\hbar\omega_s}$$

is essentially the ratio of a recoil energy to the phonon energy $\hbar\omega_s$. The recoil energy is that given to the whole crystal of N atoms of mass M by the capture of a neutron of momentum $\mathbf{p}$. For large N, the sum over modes can be replaced by an integration, and N drops out of the final answer.

In the limit of low Debye temperature, the equations can be greatly simplified, and they then yield the Voigt profile of the Bethe-Placzek theory. In a gas, the capture of a neutron leads to a recoil of the compound nucleus. The resonance energy is shifted from its ideal value by the amount R of the recoil energy. In the limit of very high Debye temperature (stiff binding), the resonance is a Lorentzian function of the expected natural width, but without recoil shift. The momentum of the neutron, but no appreciable kinetic energy, is transferred to the whole crystal lattice.

I wrote a paper describing this work and had valuable suggestions from Nordsieck, Teller, and Placzek. Then I showed it to Bloch. He made the first real criticism of the paper: the results were contained in the complicated equations given above. The usefulness of the work could be greatly increased if it were shown that the equation could be used to produce a graph. I attempted to persuade Bloch that I had obtained the interesting limiting cases analytically, but to no avail. He wanted a figure of a non-trivial intermediate case. When I went to Columbia my first research involved many days at an electric Marchant calculator. This was very tedious, but finally I had evaluated the necessary multiple integrals, and prepared some crude hand-drawn figures so that the paper[9] could be sent in for publication. One of these figures is shown here (figure 1).

I also sent in an abstract of the work[10] for the Thanksgiving 1938 meeting of the American Physical Society. I no longer remember what kept me from going to the meeting, but I had to look around for a colleague willing to give the presentation in my place. Julian Schwinger, who was a Columbia

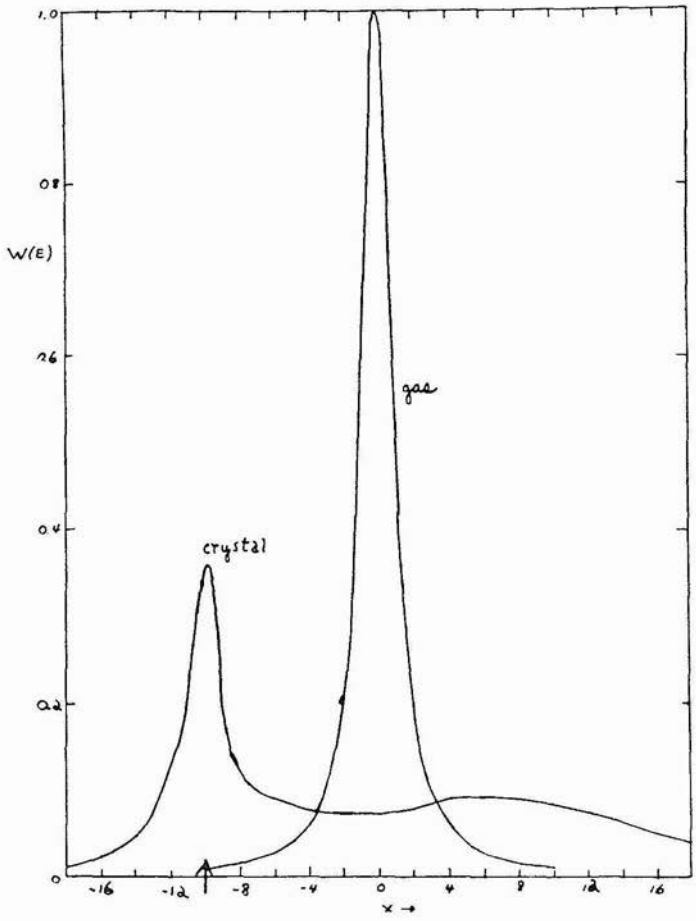

FIG. 1. PLOT OF THE NEUTRON RESONANCE ABSORPTION CURVE in cold solid silver for an assumed value of line width equal to one quarter of the Debye energy. The curve one would obtain with free atoms is shown for comparison. The abscissa measures the energy distance from resonance in units of the half width. If the lattice binding were very strong, the curve for the crystal would have the same form for the gas, except that it would be centered about the point shown by the arrow.

graduate student at that time, was scheduled to talk at Chicago on his work[11] on the quadrupole moment of the deuteron. I asked him to substitute for me, and after glancing at the paper he agreed to do so. I never got any fan mail from his talk, but had considerable pleasure in later years when Julian told me that he had learned some useful tricks from the paper, based on the fact that in a problem with a continuum of modes, it is very unlikely that two or more phonons are emitted into the same mode.

My 1938 work applied to capture of neutrons in a crystal. Some experi-

mental confirmation of the calculations was obtained[12] at Brookhaven National Laboratory. However, the observable effects were in practice relatively small because the nuclear recoil energy was not very large compared to the resonance width. It should have been obvious to me that the analysis given above would apply as well to absorption of gamma rays as to the capture of neutrons. Ordinarily the recoil energy of a nucleus during absorption of a gamma ray would be small compared to the resonance line width. In the case of transitions involving long-lived nuclear isomeric states, however, the natural width could be exceedingly small. Of course, in 1938, I had never heard of isomers. In later years many people were looking for resonant absorption in target nuclei of gamma radiation that had been emitted by the same kind of nuclei in a source. The difficulty of observation came from the recoil losses of energy on emission and absorption, together with the very narrow resonant line width. Several ingenious methods were devised to make up for the recoil energies of the emission and absorption processes. The first success in this field was achieved by Philip Moon[13] of the University of Birmingham. I met him in England shortly after moving to Oxford in 1956, and he said that we should have a talk about the implications of my 1939 paper for his work. Unfortunately, this invitation was not followed up by a meeting. Within a year, I received two reprints from Rudolf Mössbauer[14] of Heidelberg and Munich that told of his discovery and indicated that he had been referred to my old paper by one of his teachers, Hans Jensen. The graphs Bloch had persuaded me to publish gave Mössbauer the clue he needed to understand the nature of his effect.

The existence of the Mössbauer effect provides a very neat proof[15] that radiation is emitted "instantaneously." When a nuclear isomer with a lifetime of perhaps a microsecond emits gamma radiation, the emission time might be uncertain by several lifetimes, but the time taken for the actual emission process must be short compared to the sub-picosecond periods of the lattice vibrations. Otherwise, the radiative recoil of the nucleus would be stretched out over several vibrational periods, and the vibrational quantum numbers of the nucleus would not change with any appreciable probability during the nearly adiabatic emission process. (Actually, a theoretical analysis shows that the time taken is of the order of the much shorter time it takes light to traverse the nuclear radius.)

The Mössbauer emission and absorption of radiation can be thought of as simply giving a sudden impulse-like blow to the nucleus in the host lattice. This provides a very helpful picture for many important features of the Mössbauer and other radiative effects.

It was only after such considerations that I realized there was not much difference between the theory of the incoherent scattering of x rays (Debye-Waller theory) and the theory of the Mössbauer effect. The scattering of an x ray also transfers a momentum to an atom that can be replaced by an

impulsive force. (Bloch did not tell me that, although he must have known it.)

NUCLEAR MAGNETIC INDUCTION (OR RESONANCE)

During the war years 1943–1945 I worked at Columbia on microwave magnetron oscillators. In 1945 I had occasion to visit Bloch at Stanford, and he made a request that I found slightly mysterious, but was happy to grant. He wanted to have a permanent magnet of the sort used to provide magnetic fields for magnetrons. I explained that our magnetrons were rather small, and since the field had to be high, we had to attach to the usual horseshoe magnet some tapered pole pieces of high permeability iron. These increased the magnetic field, but considerably reduced the spatial extent of the strong field region. Felix wanted a larger region of strong field, so we agreed that I would send him a horseshoe magnet with pole tips removed, magnetized to the highest degree possible with our facilities. On return to New York, I hastened to get a spare magnet and to magnetize it as strongly as I could. I then sent this to Bloch by parcel post.

Even after the wonderful discovery of nuclear induction in 1946 I had no idea why he had wanted the magnet. He told me the reason some years later. The conventional wisdom in 1945 was that the time required for attainment of thermodynamic equilibrium of a system of nuclear magnetic moments in a solid was very likely quite long. In fact, Bloch and I had both heard Van Vleck lecture in the summer of 1941 on the theory that clearly seemed to indicate a very long relaxation time. In 1945, Felix was thinking of the forthcoming attempt to detect nuclear magnetism. To do this he needed to have the spins partially oriented along a magnetic field. Thinking that this might take a long time, he wanted to have a sample containing protons "soaking" in a magnetic field for a time sufficiently long for thermal equilibration so that there could be a net nuclear magnetism. As the work proceeded, Bloch realized that doping a liquid sample by paramagnetic impurities would work much better. Hence the sample of protons soaking for many months in my magnet was never used. Still, it might have been necessary. Bloch never offered to return the magnet. He did, however, say that he did not think it was as strongly magnetized as I had measured it to be. I suppose the postal authorities may have opened the package and partially demagnetized the strange steel object. Bloch did not mention this episode in his Nobel Lecture of 1952.[16] Perhaps he thought it would embarrass me!

CHEMICAL SHIFTS

About 1939, it became a matter of importance to members of the molecular beam laboratory at Columbia to know the precise value of the

magnetic field acting on a nucleus in an atom or molecule. The largest part of this is the externally applied field. However, the nearby electrons acquired an induced diamagnetic current, which produced an Amperian contribution to the magnetic field at the nucleus amounting to about 10 ppm of the applied field. This was roughly estimated by Sydney Millman and Polykarp Kusch and Rabi.[17] I made a more quantitative calculation for the case of atoms. I could solve the problem exactly for a hydrogen atom, and quite well, using the Fermi-Thomas model, for more complicated atoms. I did not try to do anything with molecules. My work was published under the slightly cryptic title "Internal Diamagnetic fields."[18] Later, Norman Ramsey showed how to make the corresponding calculation[19] for the hydrogen molecule.

In 1951, I joined the Stanford Physics Department faculty. At that time, Bloch was just discovering that super-high-resolution nuclear magnetic resonance made it possible to resolve the resonances[20] given by the magnetic moments of protons in different parts of a hydrogen-bearing organic molecule. These effects were clearly caused by internal diamagnetic fields, but were much easier to see than they had been in the earlier molecular beam experiments. The chemical shifts have turned out to be one of the most important tools for work in structural chemistry.[21] Bloch applied the method to some simple hydrocarbons, but as a physicist he did not wish to get involved with more complicated molecules. In subsequent years, John Pople[22] and others gave fruitful theoretical methods of analysis of the internal diamagnetic fields for complicated molecules. With the general commercial availability of super-high-resolution NMR apparatus, the method of chemical shifts has attained very wide and fruitful application. Certainly it is within the range of plausible possibilities that Bloch might receive a richly deserved Nobel Prize in Chemistry for his pioneering work in this field. (He also could easily have received a Physics prize for his work on electrical conductivity,[23] ferromagnetism,[24] magnetic scattering of neutrons,[25] or the first precise measurement of the magnetic moment of the neutron.[26])

LASER THEORY

The theory of an idealized two-level system has played a very important role in the analysis of all kinds of resonance physics. The theory was developed in the late thirties in independent papers by Schwinger,[27] Rabi,[28] and Bloch.[29] I used these techniques in the analysis of the hydrogen fine structure work with Retherford.[30] Following Bethe,[31] the radiative decay of the $2p$ level was simulated by adding to one of the equations a term with a damping constant.

At Stanford, I worked with Theodore Maiman[32,33] and with Irving Wieder[34] on the triplet fine structures of the $n = 2$ and $n = 3$ states of the

helium atom. The two-level theory was still useful. Decay constants had to appear in each equation, but since they were equal, the algebra was simple.

On the other hand, my work with Michael Sanders and with Lee Wilcox[35] on the fine structure of the $n = 3$ states of hydrogen involved two (and more) level problems in which the states involved had different decay constants. It was still possible to insert radiative damping terms into the equations, but the algebra became enormously more complicated for the analysis of the $n = 3$ resonances. One such calculation took me ten pages of lengthy equations.

The way out of this difficulty was pointed out to me by Bloch. I was trying to solve a transient problem, but the measurements involved a steady state of systems excited by electron bombardment and decaying through the emission of radiation. Felix showed me how the use of the density matrix techniques could lead in about one-half page to the same result I had obtained with the wave equations linear in the probability amplitudes. The density matrix equations were bilinear in the probability amplitudes, more complicated and more numerous than the amplitude equations. The observed resonance signal was bilinear in probability amplitudes, however, and hence more readily calculated using the density matrix techniques. A very important simplification came by looking for quasi-steady state solutions of the density matrix equations instead of transient solutions of the time dependent wave equations.

I later found this technique of great value for formulating my theory[36] of a laser. Density matrix methods are almost universally used now, but very few people used them in 1955 when Bloch taught them to me. Of course, I had read papers[37] by Dirac and by von Neumann, who used density matrices. The work of Landau was not then known to me. I had not been clever enough to see the potential usefulness of the density matrix, and was enormously impressed by what I assumed to be Bloch's breadth of scholarship and deep physical insight.

A few years later, however, I learned that although Bloch had known of density matrices from the 1930 era, their utility in the fifties was suggested to him by Leonard Schiff. The first and second edition of Schiff's textbook on quantum mechanics (1939, 1955) did not have a single word about density matrices. When Schiff was asked why not, he is reported to have replied something like, "I was writing a text book on quantum mechanics and I thought that density matrices were a part of statistical mechanics." The third edition, which appeared in 1969, did have a brief section dealing with density matrices.

After hearing of these things, I remembered a conversation with Llewellyn Thomas at the Columbia Faculty Club in the late forties. Thomas was teaching quantum mechanics that semester. This was a course I had taught previously, and I asked him how he dealt with the subject. He re-

plied, "I begin with the density matrix." Naturally, I wanted to know why he proceeded in such an unconventional fashion. He explained that he wanted to discuss the most general state before he considered special states. I have learned a great deal from Thomas, but usually it took me several years to understand what he told me. Incidentally, Schiff had learned his early quantum mechanics from Thomas at Ohio State.

IN CONCLUSION

The story of these encounters with Felix Bloch is necessarily very sketchy and incomplete. The five episodes were immensely valuable to me, even if I missed most of the opportunities he gave me. The reader can take it for granted they are only part of the story of my entire relationship to Felix, and that my words could never express how much this has meant to me.

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